

## Meson Team History

The first resonance particles,  $\omega$ ,  $\rho$ , and  $\eta$ , were discovered in 1961-62, causing confusion about what really was an *elementary particle*. The  $\rho$  meson, for instance, was called the *dipion* for some time. My first table of stable elementary particles was an internal Stockholm University document, dated 1960, based on Art Rosenfeld's wallet card and some other documents. Starting in 1961 new particle states proliferated from discoveries in Berkeley and Dubna, in particular because the Dubna physicists reported all peaks in every histogram with mass and width. I then updated my tables and published them in Rev. Mod. Phys. in 1963 [1] with the following warning:

Table II is a catalog, possibly incomplete, of those reported particles which are customarily called resonances. Many of these resonant states have not been generally accepted. We leave it, however, to the user of the table to put question marks against or delete such resonances which do not agree with his theory or experiment.

In an Erice summer school 1963 I had the occasion to show my manuscript to the CERN Director General Victor Weisskopf, who promptly asked me to hand in an application for a fellowship at CERN. I was accepted and I moved to CERN in the fall 1965 – a remarkable event considering that Finland was not a member state. Thus my particle tables really shaped my destiny.

For my first tables I received more than 500 request cards, something never seen before at NORDITA or in the Niels Bohr Institute. In December 1963 I published an updated and considerably enlarged version [2, 3] which began with the words

....we have omitted all such resonances, which were considered dubious a year ago, and for which no further evidence has been produced since then.

Correspondence with Art Rosenfeld led to that we both saw advantages in collaborating rather than competing. I proposed to distribute the June 1964 wallet cards at the Conference of High-Energy Physics in Dubna since Art was not going there, and that made him put my name on the UCRL-8030 team. This resulted in our first common publication [4] which began with the words:

This data survey represents the merging of two periodic compilations of data – University of California Radiation Laboratory Report *UCRL-8030* by Barkas and Rosenfeld, which has been issued several times since 1957, with accompanying wallet cards, and the tables of Matts Roos [3, 1].

The next years I worked on both stable particles and resonances, but the proliferation of new particles soon required specialization. Meson data were compiled in 1966 mainly by Art and his collaborators William J. Podolsky and Paul Söding (from Germany) and myself at CERN. One may consider this as the beginning of the Meson Team. The “8030 team” did not yet have a name, but in 1968 I proposed *Particle Data Group* for the collaboration and *Review of Particle Properties* for its publications. Only in 1996 was the latter changed to *Review of Particle Physics*.

In 1967 Bill Podolsky quit in order to do his thesis research, and Paul Söding was invited to join DESY starting october 1968. Art then suggested to transfer the whole meson compilation to Europe, with Paul and me responsible for it. Paul clearly relieved me from the growing volume of work, it was convenient to get together at CERN to do it, and, Paul being an experimentalist whereas I was a theorist with a limited knowledge of statistics, he was a particularly valuable collaborator to me. Mesons remained an interest also of Art.

At that time Claude Bricman from CERN also joined the PDG team to work on baryon resonances, and he stayed for a decade. Thus the CERN team and the Meson Team did not always overlap.

We scanned regularly more than a dozen periodicals (much more than was the custom at LBL), even if some were less important. Europeans in those days often published in national periodicals to honor their country, or perhaps because they had grants or positions which required them to do so. Paul and I established a rule that every paper had to be read by both of us, with some exceptions dictated by language – I could scan Russian papers in original (the translated journals sometimes appeared only two years later). Once the scanning and reading was done, we met at CERN to compare notes, and of course to agree on how to handle the data. In time before every Edition I spent several weeks in Berkeley to discuss complicated issues such as conflicting data, whether to accept new particles, name conventions, and to proofread the Meson Tables. We sent our results on the standardized LBL data sheets to Berkeley. I also participated in Berkeley in discussions on stable particles, which the LBL team worked on. Occasionally Paul and I had to defend the integrity of the Meson Team against LBL attempts to change our decisions or careful wordings. We felt like an independent team, whereas that was not always recognized in Berkeley.

Early on, simple averaging was done on a desk calculator or on a pocket calculator. CERN bought their first electronic desk calculators in 1965, I believe, big Friden machines with four memories visible on a small display. We felt we wouldn't gain much time by punching data onto cards and running an averaging program on a main-frame computer. When the LBL data sheets and reference sheets were introduced, they were in rigid punch card format, with 80 characters per line. This situation changed when complicated branching ratios had to be estimated, in particular those of the K mesons. In 1971 when Tom Trippe joined the PDG team in Berkeley, he gradually took over the responsibility for the K mesons. The TABSU program started running in Berkeley at the end of 1968, and was subsequently introduced in the CERN computers.

In 1969 CERN was already so impressed by the PDG work that they asked me to give a course in statistics – a task which I could not have done without a task force of four, among them Fred James and Bernard Sadoulet. A milestone occurred in 1970 when the RPP was published for the first time in a European journal, Physics Letters.

With all the discoveries of meson resonances the work load grew and the expiration of my CERN contract in 1971 was in sight, so the Meson Team had to take on new collaborators. For the 1972 Edition we co-opted Jochen Bartels, a graduate student from Hamburg, a theorist, and simultaneously Vladimir Chaloupka, a Czech fellow at CERN. The idea of the CERN directorate and myself was that Vlada should replace me, but even so he was not given a long-term contract.

To collaborate with Vlada was fascinating and spectacular because of his volcanic temper. When reading a paper with doubtful data he would explode in laughter or in four-letter words (some Czech words had more letters), completely disrupting my concentration. Paul had a very different temperament, he was calm, defended his clear opinions stubbornly, and was not willing to settle an issue by majority vote. This generally led to more careful analyses of the data at hand before a consensus could be found. Nevertheless, it should be mentioned that many of the complicated issues had no obvious solution, they originated in bad experiments and conflicting data. For instance, the A2 split and the Missing Mass Spectrometer bumps S and T were controversial and our handling caused severe criticism by the authors.

When my staff membership at CERN expired in 1971, and I left for Helsinki, CERN continued to be the European center of PDG and the center of the Meson Team. Starting 1972 I made short handwritten notes on every paper read, often more than 100

notes per Edition, that I made available to those of my collaborators who took the trouble to copy them. I record some correspondence with Tom Lasinski in Berkeley in 1973 on the interpretation of Breit-Wigner resonances as pole positions on unphysical sheets. Another short-time collaborator from LBL in the Meson Team was Denyse Chew who visited CERN in the summer of 1974.

Paul Söding left the team in 1975, and Vlada had moved to SLAC but continued the collaboration from there. The 1976 issue of the Meson listings were prepared mainly by the CERN fellows Richard J. Hemingway and Michael J. Losty from Great Britain, but also by Vlada and myself. Richard and Mike started the tradition of celebrating a collaboration meeting by going to a restaurant in St-Jean-de Gonville.

At this time the CERN directorate realized that PDG at CERN had to be led by a permanent staff member, or else Europe risked to loose its share in PDG. Lucien Montanet was then appointed to be the administrator, he joined the Meson Team because mesons was what interested him, and from now on he assumed the responsibilities to find new Meson Team members when needed, to finance the printing and some of our travelling, to take care of running the programs TABSU and AHR in the temporary absence of everybody else, and not least to organize the team dinner in St-Jean-de Gonville. On Lucien's proposal the Meson Team in 1976 -1980 was enlarged with the Italians Carlo Dionisi and Mirco Mazzucato, with Lucien's secretary Karin Gieselmann who helped with data inputting, and with George Yost who handled the verifications of meson data in Berkeley. Working with Italians was a new experience, easy-going like *Così fan tutte*, the discussions required many cups of espresso, and the collaborators occasionally arrived one week late.

From 1977 Richard J. Hemingway and Mike Losty were still on the team, although Mike had moved to LBL. I believe Mike would have liked to see the meson activity moved altogether to LBL – clearly a political issue unacceptable to CERN.

By now the data processing procedures had to be modified. Not everybody could read every paper (only I did), instead we tried to assign specific particles to everyone. This was not as simple as it sounds like, because some papers contained information on many particles. One needed a master list of papers, and depending on their particle content they were assigned to a specific reader. The rule that each paper had to be read by at least two people still applied, because I continued to read every paper until 1994.

Copying was cumbersome in those days, inputting text into computers was hopeless because it had to be done by punch cards. With the advent of the VAX-VMS operating system I started in 1988 to type my notes into computer files so that everybody could have a copy.

In 1981 Lucien persuaded the Spanish CERN research associate Manuel Aguilar-Benitez to join the Meson Team, a very fortunate choice because Manuel was to become a really longtime and reliable collaborator as well as a close friend. From now on an increasing number of Europeans worked with the PDG on various other sections than mesons.

The Swedish CERN fellow Christian Walck joined the team until 1983, Frank Porter at Caltech became a most careful internal referee and proofreader, Mike Losty moved to Ottawa, but stayed on the PDG team until 1984, and Nils Törnqvist from Finland at that time agreed to join the team. Since Lucien was an Editor of Physics Letters B, he often had very first-hand information, and he wanted to discuss every experiment in detail. Lucien was a very kind person, liked by everybody. Unfortunately he is no longer among us.

In 1986 two more collaborators joined us: Klaus Schubert from Germany, and Juan J. Hernandez from Spain, the latter still active in the team. In 1987 the Italian Carlo Caso approached me with a request to join the Meson Team (too late for the 1988 Edition) – the first time this had happened - and in 1992 the Russian Simon Eidelman and the Indian Atul Gurtu did likewise; all three are still active. Atul joined specifically to integrate LEP results on Z and W mesons, and Carlo joined him to form a Z/W subteam working quite independently of the rest of the Meson Team. Klaus Schubert resigned after the 1990 Edition. During the years 1987-1993 Carlo Caso, Gianni Conforto who worked with Stables, and myself formed a group supported by INFN in Florens, in practice however, we were located in a modest barrack at CERN.

The Missing Mass Spectrometer peaks T and U again came up in 1987 with charges of misnaming and mistreatment. We nearly went to court, but a compromise how to treat those data was finally found.

Over the years, several physicists had worked on the statistical methods we used: Gerry Lynch in 1987, George Yost and Klaus Schubert the following years, Frank Porter for a decade, and Fred James in 1994. After Lucien's resignation in 1994 from the

responsibility as PDG administrator at CERN, there followed some political maneuvering to find a successor. We refused to accept some proposed, entirely extraneous people, but Manuel found a solution by persuading Fred to take over, and CERN fortunately accepted it. Fred and I had worked together for years on MINUIT, we had also written a book on statistical methods which originated in my 1970 lectures at CERN, and we are great friends.

In 1994 CERN assigned us an own secretary, Flic Nicolson, and working space in a trailer with no water nor toilet. A year later we finally got decent offices, one for Flic and two for the team members. We then also got generous support for per diems, travel costs to Berkeley, computers, and salaries for three persons in addition to Fred (Flic, Simon, and a programmer working with Fred). Michelangelo Mangano joined with the task of SUSY searches

Occasionally the Berkeley team and the Meson Team got into heated disputes which could be termed power struggle. The Meson Team saw itself as an equal partner which could make independent decisions, whereas some of the LBL team chairmen considered us as subordinate, whose decisions on meson matters could be changed or voted on in Berkeley. I was certainly the most arrogant defender of the European view. I should mention that Frank Porter handled meson data with marvellous diplomacy and neutrality.

When I resigned from the Meson Team in 1995 because my interest had shifted from mesons to neutrinos and from neutrinos to cosmology, nobody wanted to take over my job of reading all papers, writing notes on them, encoding data into the Berkeley data base, coordinating everybody's work, supervising all computer fits, keeping contacts with Berkeley, and representing the Team in Berkeley – the last time for me was in 1996. I counted that I had spent 2-3 months' full-time work on the Meson listings every year, and I nearly always went to Berkeley to proofread the final tables. Thus the work had to be reorganized among Manuel Aguilar-Benitez, Juanjo Hernandez, Carlo Caso, Nils Törnqvist, Simon Eidelman, Atul Gurtu, and the two new Swiss members Claude Amsler and Michael Doser.

The journals to be read were divided among the members and Claude assumed the rôle of a coordinator, but without the heavy obligation of reading every paper and writing notes. Fred resigned in 1999, and Michael Doser took over as the Meson Team representative in the PDG Management Board in 2000.

The team noted in a memo from Claude in November 1999:

“All encoding work for the 2000 Edition has been done by Betty. ...This is a bottleneck which complicates encoding at CERN. ...The compilation is hampered by a too rigid format inherited from a previous generation of data handling technology. ...There has not been any attempt to rewrite the programs from scratch. The very presentation of the physics is hampered by a too rigid format. The assumption on which the system is built is that all one wants to know about a meson is its mass, width and branching ratios/partial widths, and that a blind average will do.

Gradually we have had to introduce new characteristics for some mesons, making them all look rather individual. Some mesons became so individual that they had to be treated entirely differently (e.g. the eta, the W, the Z), but that trend is visible throughout: we need T- and K-matrix pole positions, C-nonconserving decay parameters, D/S –wave amplitude ratios in decay, etc. ...The most individualistic mesons require minireviews where we really explain ... what is known and what is contradictory and what impact do the newest data have.”

This memo then went on with a proposal for a radical solution, of which I don't know to what extent it has been implemented.

Obviously the work in The Meson Team has always been considered interesting and worthwhile, since it always was easy to recruit new members, many even came on their own initiative.

The 2004 Edition still listed me as an author, although I was not active anymore, not even in neutrinos. This fact, and the invitation to the present meeting I consider to be a great honor.

[1] Matts Roos, *Rev. Mod. Phys.* **35**, 314 (1963)

[2] M. Roos, *Phys. Letters* **8**, 1 (1964).

[3] Matts Roos, *Nucl. Phys.* **52**, 1 (1964)

[4] Arthur H. Rosenfeld, Angela Barbaro-Galtieri, Walter H. Barkas, Pierre L. Bastien, Janos Kirz, and Matts Roos, *Rev. Mod. Phys.* **36**, 977 (1964)

[illegible]